Noesis

The Journal of the Hoeflin Research Group
(Issue 27, June 1988)

Editorial

Ronald K. Hoeflin
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More renewals: Cedric Stratton and Marilyn vos Savant have indicated their interest in continuing as members for the coming year, which brings to 13 the total number of continuing members. The only members who have not renewed are Eric Hart, William Hacker, Jeff Ward, and Karl Wickman. It is possible that one or two more will renew eventually, but I have stopped sending issues of Noesis to these individuals until I hear from them.

The July 4 meeting: I currently expect 5 members plus myself to participate. These 5 are Chris Cole, Dean Inada (if Chris brings him), James Hajicek, Keith Raniere, and Ray Wise. It is remotely possible that one or two others will also join us at the last minute. Of these six anticipated participants, I believe I am the only one who is not employed in the field of computers.

Anyone who wants to attend at the last minute may phone me at (212) 582-2326 for information.

This issue was to have included one-page synopses of papers that participants would be giving at the meeting, but aside from Chris Cole's paper on Bayes' theory of probability in the preceding issue, no further synopsis has been received. Any that do will be published in the July issue, which will be sent out before the end of June, probably (Bayes willing).

I believe that the Brooklyn Hall of Science has on display the world's first quantum-mechanically accurate model of a hydrogen atom, which was constructed at a cost of $40,000. Throwing a switch is supposed to send it into progressively higher energy states, with the probability distribution of the position of the electron changing from one state to the next. Precisely how this can be shown in a three-dimensional model is not quite clear to me, but I gather that lasers cause some sort of vibrating target to glow in the appropriate fashion. This exhibit might be worth going to see, if the other participants are interested.

The Titan Test: Scot Morris, the puzzle editor of Omni magazine, sent me a reworded version of my Titan Test a couple of weeks ago and asked for my comments. I believe he was trying to make the wording a bit briefer so as not to impose too much on Omni's space limitations, but the shortened wordings he proposed generally left the problems less clear. But at least he is at last moving ahead with the test, and I was glad of the chance to go over the test again and make some last-minute improvements.

(The envelope bears the postmarked date: May 18, 1988, in case it is illegible below.)

David Geiger
1003 W. Ridge
Mqt, MI
49855
Marquette

Titan Society
P.O. Box 7430
New York, N.Y.
~~10116~~

(Editor's note: The following comment appears on the back of Mr. Geiger's envelope.)

I think the answer is no.

Dear Ronald K. Hoeflin

I have a question for the members of your organization.

Is there ever going to be another problem like the four color problem.

David Geiger

I am 60 years old.

(Editor's note: I never heard of Mr. Geiger before, nor am I sure what he means by "another problem like the four color problem." But see the next two pages for one possibility.)

THE NEW YORK TIMES, WEDNESDAY, MAY 25, 1988

Crack the Problem; Win Fame

By Martin Gardner

HENDERSONVILLE, N.C.

A most appealing irony of modern technology is that generations of intellectuals, schooled in the discipline of higher mathematics and equipped with supercomputers, nonetheless have been stumped in their efforts to solve a seemingly simple 357-year-old number puzzle.

The theorem, put forth by Pierre de Fermat, a 17th century French mathematician, has teased the brains of thousands of mathematicians.

Having eluded a "proof" for more than three centuries, Fermat's Last Theorem, as it has become known, has thus taken on a kind of mystical importance. Anyone who solves it will be instantly famous, as became clear from the attention given a Japanese mathematician when he offered a "solution" earlier this year. Experts determined last month that it was flawed, however, so today the theorem continues to beckon.

Perhaps the most frustrating aspect of this puzzle is that it is so easy to understand: Does the equation $a^n + b^n = c^n$ have a solution in which *a, b, c* are positive whole numbers and *n* is greater than 2? (When *n* = 2, for instance, there are many solutions, including $3^2 + 4^2 = 5^2$, or $9 + 16 = 25$. Above the second power, however, no whole numbers seem to work. Proving that this holds true for all other numbers is what has baffled the giants of math.)

Martin Gardner is author of numerous books on science and mathematics. He wrote the mathematical games column in Scientific American magazine from 1957 to 1982.

As if to further tantalize, Fermat scribbled in the margin of an arithmetic book a note in Latin saying he had a "remarkable proof" that there were no other numbers for which the equation would work, but the margin was too narrow to contain it.

Fermat never published his proof, probably because he soon discovered it was unsound. Yet this enigma continues to capture the imagination of mathematicians around the world.

Even armed with computers, today's scholars have not been able to find a counter example to prove the equation false. Tens of thousands of papers have been written about the problem. Frustration over the task has even invaded two works of fiction: "The Devil and Samuel Flagg," a fantasy yarn by Arthur Porges, and "Murder by Mathematics," a mystery novel by Hector Hawton.

Hundreds of erroneous proofs have been established, some by top mathematicians, and even today more amateurs exhaust their energies on the problem than on trisecting the angle. When David Hilbert, one of the world's great mathematicians, was asked why he never worked on Fermat's Last Theorem, he replied, "Before beginning I should put in three years of intensive study, and I haven't that much time to squander on a probable failure."

Although it is almost certain that no amateur will solve the theorem, there is always the nagging possibility that one might. I am only a mathematical journalist, but every year I receive many such "proofs." I promptly return them unread, my conscience slightly twitching, especially when they come in handsome, privately printed brochures.

The University of Chicago's mathematics department once had a form letter stating: "We very much doubt that any treatment as simple and short as yours is likely to provide a solution. . . . Should you wish a careful analysis of your solution, we would be able to provide it only upon provision of a suitable fee."

The German mathematician Edmund Landau had a form letter that read: "Dear Sir/Madam:

Your proof of Fermat's last theorem has been received. The first mistake is on page ———, line ———."

The blanks would be filled in by a graduate student.

I have also heard of a mathematician who closed his form letter with, "I have an elegant refutation of your attempted proof, but unfortunately this page is not large enough to contain it."

An American expert likes to return crank proofs with a note saying he is not competent to evaluate them, but so-and-so is. He then provides the name and address of another crank who thinks he has a proof.

Most mathematicians are convinced the theorem is true and eventually will be proved. A minority suspect it is false but believe that the simplest counter example involves values of *a*, *b* and *c* that have millions of digits. To establish the theorem, it is only necessary to prove it for prime exponents (primes are numbers other than 1 that are divisible only by 1 and themselves), but already it is known that the theorem is true for all exponents smaller than 125,000.

Fermat's puzzling theorem, which is 357 years old.

I belong to a whimsical third group of people who believe and hope the theorem is undecidable. The mathematician Kurt Gödel, in a celebrated paper, showed that arithmetic contains statements that cannot be proved true or false within the formal system of arithmetic. If Fermat's theorem is false, there must be a counter example, but of course its existence would make the theorem decidable. It follows that if the theorem is Gödel-undecidable, it must be true.

This leads to a dismal (though to me delightful) possibility. Mathematicians and their supercomputers will forever struggle with the theorem, never finding a counter example, never knowing for sure if it is true, and never giving up because, like the mountain, it's there. ☐

May 23, 1988

Cedric Stratton
P. O. Box 60111
Savannah, GA 31420

Dear Ron,

Thank you very much indeed for maintaining my currency in your group in spite of my recent low-profile participation. In response to your letter of 6th May:

(A) I will be happy to send you $20.00 when I have my pay-check next week. I have been financially strapped for a few months.

(B) Whatever I write and send you for the Noesis I consider a privilege, or duty, of membership.

(C) I probably will not be able to visit New York on the weekend of July 4th. I have expectation of a part in a movie to be made in Savannah at that period. But I plan to prepare a message to be used as evidence of presence in spirit, if you would care to use it.

To bring you up to date on my recent activities:

I believe I mentioned I re-married last June. I bought a new house in August and moved in—it needed (and still needs) a good deal of repair work. That's why I was able to buy it at about half market-price of a well-kept home.

I am writing a (low-level) text-book for my largest group of students and plan to offer it to publishers shortly, so I have been writing a proper synopsis for it and polishing it for the selection editor.

During the past 8 months I have been bearing a double student-load because of the demand for my courses (not MB!, but the courses themselves, which are pre-requisites for others). Finally things are beginning to wind down.

In January, just after I got back from a four-week trip to England, my car was totalled. My new wife was shaken up but had a few bruises as well. Surprisingly, that was the only personal injury. In spite of full insurance cover by the other party's insurance company, I still wound up out of pocket because the age of the car placed the largest deficit between the amount I still owed and the cost of replacement!

I will be putting some thoughts together on things you might use in Noesis.

Yours very sincerely,

Cedric

P.S. I hope, when I have finally either (a) got my book accepted or (b) not got my book published, that I shall have some time to look at the many alternate tests you sent out starting about 18 months ago. Are you still interested in my input, or do you have your tables and statistics complete?

(Editor's reply: I suggest you wait and try the final test, the Titan Test, which consists of 48 problems culled from the six trial tests. It is undergoing some final modifications currently.)

May 23, 1988

(Address deleted
by request.)

Ronald K. Hoeflin
Post Office Box 7430
New York, New York 10116

Dear Ron:

Thanks for your letter of May 15.

I'm eager to see your new test appear in OMNI. If you think of it, will you let me know the date of issue so that I can buy a copy for myself? I guess the unlisted phone number is a good idea. Did anyone telephone you inquiring about Kevin's test? I still get mail about that, myself.

It is not surprising to me that Scot's alterations make the test worse in subtle ways. What is surprising to me is that someone who is not thoroughly knowledgeable about it would consider making any changes whatsoever. And who should know more about the test than you?

I do want to continue as a member of the Hoeflin Research Group, but feel I have lost touch with my situation on dues. I had thought I was a member until March of 1989. Am I wrong about that?

I'm curious to know more about your new girlfriend. What's she like?

If you have time to write, do.

Sincerely,

Marilyn

Marilyn vos Savant

P. S. Don't publish my address or telephone number, however.

My mail is becoming insane.

C. M. Langan
P. O. Box 131
Speonk, NY 11972

Dear Mr. Hoeflin:

I'm writing to try again to clarify my remarks on your marble problem (#26 on "Trial Test B"). However you might construe my comments on the other points I addressed in my previous letters, the ones concerning this problem are consistent with your own position.

You seem to have assumed, perhaps because you consider the problem a dead issue, that my position parallels that of members Inada and Cole. Actually, our respective views are contradictory. When I wrote that I "see nothing the matter" with #26, I meant that your original formulation, as reproduced in Noesis #14, is complete as it stands and requires no revision. Irrespective of your own *answer* to this problem - if it is 67%, it is correct even by direct application of Bayes' theorem itself - you provided enough information to enable its full solution. To your credit, it is *your* position which is classically Bayesian. In fact, if you actually knew nothing of Bayes' theorem at the time you designed and solved the problem, you probably succeeded in applying what can be described as an independent derivation of it.

The other members' remarks amount to the statement that #26 is unsolvable as it stands, yet there is nothing preventing a real situation for which your initial formulation is an exact description. So they've taken the position that probability theory offers no clear means by which to cope with this facet of reality. It can readily be shown how and why such a view is mistaken.

Mr. Inada cites a paradox based on the reasoning of Laplace, who originally formulated a tenet of inductive probability called the "principle of indifference" (the strong form of which is due to Harold Jeffreys: "If there is no reason to believe one hypothesis rather than another, the probabilities are equal.") It is the improper use of this principle, and not the wording of your problem, which engenders paradox. I thought I'd made this clear, but I must have been too brief. I hope this letter serves as a foglight.

Frankly, I'm a little curious to see whether any of the contributors I mentioned will bother to try to substantiate their positions. The mildly confrontational tone of my previous correspondence was partly calculated to enhance that possibility. It was my thought that such a dialogue might not only be of intrinsic interest, but have a salutary effect on interest levels within the group and towards it from without.

You don't mention having received the $14 check I sent you a couple of weeks ago. If you still don't have it, write me.

Sincerely,

C M Langan

S. Woolsey
Box 1942
Houston, TX 77251

May 20, 1988

Ronald K. Hoeflin, Ph.D.
Box 7430
New York, New York 10116

Dear Dr. Hoeflin,

In the Titan Test which appears in the March-April 1988 issue of "Vidya", it seems that the answer to problem 47, the one about the black and white marbles, is impossible to determine unless a hidden assumption is made explicit.

It says "Each marble is either black or white.". But it does not say whether each individual marble has an equal probability of being either black or white. This leaves open the possibility that the colors of the marbles are chosen in such a way that one color is more likely to occur than the other color.

However, if it is made explicit that each individual marble has a fifty-fifty chance of being black or being white, then (on the average) one would expect that out of 2^{10} such black boxes containing randomly chosen marbles, for every n (where n = 0, 1, 2, 3,...,10) the number of boxes containing exactly n white marbles is $\binom{10}{n}$. (Those are the values that would be approached as one considers many independent sets of the randomly selected boxes and each set contains 2^{10} boxes.)

Then, since the probability of drawing ten white marbles (with replacement) from a box of ten marbles, n of which are white, is $\left[\frac{n}{10}\right]^{10}$, the probability that a box from which was drawn (with replacement) ten white marbles contains all white marbles is

$$\frac{1}{\sum_{n=b}^{10} \binom{10}{n} \times \left(\frac{n}{10}\right)^{10}} = .07019\ldots$$

(where b is either 0 or 1; it doesn't matter which: both give the same answer.)

(The numerator, 1, represents the probability that one draws ten white marbles from the one box (out of 1024) which contains all white marbles.)

Therefore, the answer to problem 47, to the nearest percent, is 7 %.

On the other hand, if it is done by a different method then one gets a different answer. If a number w is chosen completely at random from the set of numbers 0 to 10 inclusive, and it is posited that there are exactly w white marbles in the box, then (upon drawing out ten white marbles with replacement) the probability that the box contains all white marbles is $\left(\frac{1}{\sum_{n=b}^{10}\left(\frac{n}{10}\right)^{10}}\right) = .670\ldots$ (4 ....) thereby giving to problem 47 an answer (to the nearest percent) of 67%.

For a determinable answer, please make the hidden assumption explicit. S. Woolsey

Remarks on Newcomb's Paradox

Dean Inada
23333 Ridge Route Drive
Apt. 51
El Toro, CA 92630

First, I would like to announce to any unerring predictors that, if presented with Newcomb's paradox, I would *definitely* pick only the $1M.

Skeptical predictors may question my sincerity, since, once the being has prepared the boxes, I have nothing to lose, and $1K to gain by picking both. (Creditors are sometimes similarly skeptical)

In the creditors case, our society has developed systems for enforcing promises. Since a level of trust and dependability tends to make transactions more convenient, and Pareto optimality more attainable (c.f. prisoners dilemma) You may object to the loss of free will, but it is sometimes advantageous to suffer certain restrictions on ones actions in order allow another to predict them in exchange for some benefit. Most of these systems rely on adjusting perceived payoffs by expectations of repeat transactions, so this may be of limited use if the being's offer comes but once.

One possibility may be to take advantage of existing external enforcement mechanisms. For example I might bet a friend $2K that I would only pick the one box. If this bet serves to convince the being to put the $1M in the the box, it behooves me to make such a bet. Even if I must pay my friend $4K to convince him to make it.

Even without a friend, or contract laws, it may be possible to introject their discipline. If the being is reliable, a good way to convince it of your intentions is to be convinced yourself, if you can, for richer or poorer. One need not believe that the $1M will magicly vanish if we change our mind. (Although, if you can convince yourself of this it may be quite profitable to do so. If you believe in CPT you might want to consider reverse causality, with sqrt(1/2)M$ in the box before it is opened. Or perhaps a hypnotist might be useful.) It is sufficient to believe in commitment. Such commitment need only be strong enough resist an offer of $1K. Anyone who has ever returned a lost wallet, or honored a verbal contract, or broken, or acquired a habit is probably capable of keeping such a promise to oneself.

If I am fallaciously deluding myself into throwing away $1K, at least it will be profitable, if the being predicts I am doing it. Anyway, an unresolved paradox can make me more uncomfortable than losing $1K. And seeing the being fail may be worth $1K, but probably not $1M

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Dr. Ronald K. Hoeflin
P.O. Box 7430
New York
N.Y. 10116

20th May 1988

Dear Dr. Hoeflin,

I take it from your letter of 17th March to Alan Russell that you have not had a reply to your letter to Norris McWhirter regarding the Hoeflin Research Group.

Can I start by explaining that Norris McWhirter has now retired (though he still keeps in touch with us) and Alan Russell has now resumed his career in television.

I enclose a new wording which I propose using for the second paragraph of the I.Q. entry.

For the time being I don't want to make any other changes to the entry but I would like documentation on Alicia Witt and Keith Raniere's I.Q. test at age seven. I would also like to see the estimates from Genetic Studies in Genius.

I take your point about childhood I.Qs. I suffered from this myself. Having run up a prodigious score at age four, I then had to spend several miserable years watching the I.Q. (though not the mental age!) decline. However, I think we'll leave this for the moment.

Thank you very much indeed for your help in this.

Yours sincerely

Donald McFarlan

DONALD McFARLAN
The Editor

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The most exclusive ultra high IQ society is the Hoeflin Research Group, New York, USA, whose 17 members have an IQ which occurs at a 1 in 1 500 000 level in the population. The highest score in the admission test devised by Ronald K. Hoeflin, is 46 out of 48 by Marilyn vos Savant (see above), making her, with Eric Hart (b.1956) and Keith Raniere (b.1960), both of New York, one of only three members who have scored higher than 45 which represents a 1 in 10 000 000 level.